Economic returns to education

An empirical study from Zimbabwe

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Summary:

This thesis takes a closer look at the effect of education on the living standards in Zimbabwe. I have used the wealth index constructed by Measure Demographic and Health Surveys (DHS) to see how education affects wealth. Looking at the relationship using a simple OLS is likely to produce unreliable results due to omitted variables. In order to look at the causal effect, I will reduce these issues by taking advantage of the timing of a post-independence education reform. I will use the strategy of the regression discontinuity design and use whether or not an individual was above or below secondary school age when the reform was implemented as an instrument for education level.

I find the education reform to have a significant and large effect on school attendance. Furthermore, for the population en masse I find a positive effect of education on wealth. Looking at the heterogeneous effects among different subgroups of the population, I find that the effect of education on wealth is significantly stronger for the rural and the rural female population than for the population en masse. For the female and the urban population I do not find a significant increase in wealth due to education. Surprisingly, I find suggestive result of a negative effect of education on wealth for the rural female population.

I go on to look at employment variables to try to detect possible mechanism behind the results. For the population as a whole I find a positive effect of education on the probability of being employed. Looking at the type of employment I find education to increase the probability of formal employment in the sense of getting paid in cash. This effect is stronger for females then for men. However, I find education to increase the probability of working on own land which is associated with informal employment and lower wealth. I also fail to find an effect on the probability of being self-employed.

It is important to note that I, for wealth, did not find statistically significant differences between the 2SLS and OLS estimates, and therefore chose to interpret the OLS estimates as being causal.

The positive effects on wealth and employment are in line with previous conducted research from similar developing country contexts.

Acknowledgement

This thesis marks the end of my studies at the University of Oslo. Recognizing that the probability of ever winning an Oscar is not statistically significantly different from zero, I want to take this opportunity to thank the people who have inspired, motivated and helped me through my studies. [1]

This thesis would never come to be without the great guidance of my supervisor Andreas Kotsadam. I would like to thank him for helping me sort out my thoughts of what I want to write about and helpful remarks to the bitter end. Particularly, I would like to thank him for his inspiring and enthusiastic encouragement that made this a great process.

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Finally, I want to thank my grandmother for showing me that strong will makes all the difference!

Any remaining errors and inaccuracies in this thesis are my own responsibility.

[1] Thanks to Ida Kroksæter for making me aware of this and for, as the rest of Stensgata 1A, let me have the sofa when needed.

Contents

1 Introduction

During the post-independence era there has been a large expansion of basic education in Sub-Saharan Africa. From 1970 to 2010, the average years of schooling in the Sub-Saharan countries increased from 2,04 to 5,23 (Barro & Lee, 2013). Whether this increased attainment in education causes an increase in living standard is a basic concern for development economists (Duflo, 2001). The economic return is interesting in itself. Furthermore we know that wealth often has a large affect on other characteristics such as health (Measure Demographic and Health Surveys (DHS), 2013b).

There is a large body of literature investing the returns to education. However, most of the existing studies look at simple correlations between wage and education (Duflo, 2001). This omits the importance of family and community backgrounds, and the estimates might be biased. I will take this into account by taking advantage of an educational reform in Zimbabwe that increased the secondary school attendance.

Other papers looking at this in a developing country context, find positive effects on wages and employment from education. Duflo (2001) looks at a large primary school expansion in Indonesia in the mid 1970s. By using the variations in schooling generated by this policy as instrumental variables for the impact of education on wages, Duflo (2001) finds that estimates of economic returns to education ranges from 6.8 percent to 10.6 percent. Borkum (2009) looks at a change in grade structure in Botswana that led to an increase in education with about 0,62 years for the group affected by the change. As a consequence of the increased education, Borkum (2009) finds that the individuals affected enjoyed significantly higher labor force participation. The individual affected were also select into occupations requiring a higher skill level and benefit from higher wages. Keats (2012) finds results in line with this, taking advantage of a national reform that eliminated the primary school fees in Uganda. Keats (2012) finds that educated women are more likely to have better jobs and they are wealthier. Conditional on working, educated women are 27 % more likely to work for cash and are less likely to be self-employed. This findings are consistent with what Ozier (2011) finds in Kenya. The probability of being admitted to government secondary school rises sharply at a score close to the national mean on a standard 8th grade examination. Ozier

(2011) takes advantage of this discontinuity and find a reduction in low-skilled self employment and an increase in formal employment for the students above the threshold compared to those below the threshold who did not attend secondary school.

Agüero and Bharadwaj (2012) and Agüero and Ramachandran (2012) have looked at the same reform as I use to examine the effect of education on health related outcomes and the intergenerational transmission of schooling. Agüero and Bharadwaj (2012) find that women with more schooling engage in HIV-preventing behavior by having fewer sexual partners, and know more about how HIV spreads. Surprisingly, this does not lead to a significant difference in the effect of education on HIV status. Agüero and Ramachandran (2012) find that one year in mother's and father's schooling increases the child's schooling with, respectively, 0.049 and 0,14 years.

In contrast to Duflo (2001) and Ozier (2011), I use a wealth index created by the DHS to look at the economic returns to education instead of wages. The index is calculated using easy-to-collect data on a household's ownership of selected assets, such as televisions and bicycles; materials used for housing construction; and types of water access and sanitation facilities (Measure Demographic and Health Surveys (DHS), 2013b). Using this index instead of wages allows me to look at the effect not only for those who are formally employed. This is especially valuable in a context were formal employment is low and lack reliable data on income and expenditures.

To find the causal effect of education, I take advantage of the exogenous variation in education level created by an educational reform in 1980. I use this exogenous effect in a regression discontinuity analysis to identify the causal effect. Throughout the paper, I examine the heterogeneous effect of education by looking at different subgroups of the population in addition to the population as a whole. After finding the effect, I examine possible mechanisms. To do this, I look at the possible differences in compliers and effects of education on other variables that might affect wealth.

The remainder of this paper is organized as follows: the post-independence education reform and its contexts are presented in section 2. Section 3 describes the data and variables I will use. In section 4 I present the empirical strategy. The results from the analysis are presented in Section 5. Section 6 concludes.

2 Reform

In colonial Rhodesia[2] 25% of black children never entered school, only 33% finished primary school, and only 6% attended secondary school (Riddell, 1980, as cited by Nhundu, 1992, p.79). These low rates were due to colonial education policies aimed at reducing the black Rhodesians' "irresponsible" demand for education and controlling the rate of black education advancement. Paradoxically these policies, according to Banana (1981), created an even greater demand for education by blacks. Black Rhodesians began to view education as the only route for black upward mobility (cited by Nhundu, 1992, p. 80). As a response to this, one of the principle educational policies of the 1980 election manifestos of the ruling Zimbabwe African National Union (Patriotic Front) Party [ZANU(PF)] was to implement free and compulsory primary and secondary education for all Zimbabwean children regardless of race, sex, or class. Several post-independence policy initiatives designed to equalize educational opportunities was introduced in Zimbabwe in 1980 (Nhundu, 1992). "The key initiatives of the new government include (1) introduction of free and compulsory primary education; (2) the removal of the age restriction to allow over-age children to enter school; (3) the encouragement of community support for education; and (4) automatic schooling progression, especially from primary-grade schoolchildren entering secondary school" (Nhundu, 1992, p. 80). The reforms led to a 950.5% rise in enrollment in secondary school from 1979 to 1989.

It is this drastic increase in secondary school attendance I will use in the further examination of the causal effects of education. More information about the reform and about who was affected is given in the empirical strategy and results sections.

[2] Zimbabwe was known as Southern Rhodesia until political Independence in 1980.

3 Data

In this section I will present the data I will be using in the analysis and describe the variables used. The data I will use in this paper come from the Demographic and Health surveys (DHS) of Zimbabwe. Demographic and Health Surveys (DHS) are nationally-representative household surveys that provide data for a wide range of monitoring and impact evaluation indicators in the areas of population, health, and nutrition. Standard DHS Surveys have large sample sizes (usually between 5,000 and 30,000 households) and typically are conducted about every 5 years, to allow comparisons over time (Measure Demographic and Health Surveys (DHS), 2013a). I will use data from the survey rounds conducted in Zimbabwe in 1988, 1994, 1999, 2010 and 2005. The data contains individuals who were of age 15-49 (females) and 15-59 (males) in the years of the survey round). It is not the same individuals who are interviewed in each round, so I don't have a panel data set but multiple cross-sectional data sets. Not all variables are included in all survey rounds. All survey rounds will therefore not be included in all regressions.

Each survey round includes the year of birth of the individuals interviewed. I use this to calculate the age of the individuals in 1980. Each round also includes the completed education for the individuals in the year of the survey round. This allows me to relate the age in 1980 to the completed years of schooling for the individuals interviewed. I will use this to see how the reform affected secondary school attendance. How this is used in the identification strategy is explained in more detail in the next section.

The variable that I will start out by looking at is the wealth index. This variable is included in the 2005 and the 2010 survey round. "The wealth index is a composite measure of a household's cumulative living standard. The index is calculated using easy-to-collect data on a household's ownership of selected assets, such as televisions and bicycles; materials used for housing construction; and types of water access and sanitation facilities"(Measure Demographic and Health Surveys (DHS), 2013b). Each household asset for which information is collected is assigned a weight or factor score generated through principal components analysis. The resulting asset scores are standardized in relation to a standard normal distribution with a mean of zero and a standard deviation of one. Each household is then given a standardized score for each asset depending on whether or not the household owns the asset. The scores for all the assets are the summed up for each household and the individuals are ranked according to the total score for the household in which they live. Since the wealth score is made relative to the rest of the population, the scores are not comparable between surveys. The relative wealth score is also important an aspect of what I will be looking at in the following. It is important to keep I mind that any effect of education that I find, is relative to rest of the population. It is not an effect on wealth in absolute terms.

When looking at possible mechanism behind the effect education has on wealth, I will look at employment outcomes. The data sets include information on type of employment, type of employer and form of earnings. I will in particular use the variable indicating if an individual has worked for the past year as a measurement of whether or not an individual is employed. Furthermore, when examining the effect education has on type of employment and employer I will use the variables indicating whether or not an individual is being paid in cash, working on own land and is self-employed. Being paid in cash is associated with more formal employment (Keats, 2012) while both working on own land and being self-employed is associated with less formal employment.

As stated above, the wealth index is a household variable. It is therefore be important in the analysis to control for marital status. All the survey rounds include five categories of marital status; currently married, currently cohabitant, widowed, divorced, and separated. These variables will be taken in as controls in all regression.

Summary statistics are presented in Table 1.

Table 1A Summary statistics

	Whole population					Treated group Age 6-14 in 1980)		Untreated group (Age 15-22 in 1980)		
	Mean	Min	Max	sd	Count	Mean	Count	Mean	Count	Difference
Wealth score	21612.92	-140617	395551	95467.52	8933	24757.27	5809	15766.07	3124	8991.196***
Wealth index	3.099966	1	5	1.449622	8933	3.134274	5809	3.036172	3124	.0981028**
School years	7.749691	0	21	3.675574	19428	8.639901	12180	6.253725	7248	2.386176***
Secondary educ.	.5234005	0	1	.499465	19444	.6459102	12189	.3175741	7255	.3283362***
Worked past year	.6511731	0	1	.4766142	15813	.6507635	10019	.6518813	5794	-.0011177
Cash paid	.3969033	0	1	.4892744	13046	.4096471	8417	.3737308	4629	.0359163***
Work on own land	.490573	0	1	.5000036	2705	.4728295	1601	.5163043	1104	-.0434749*
Self employed	.2336678	0	1	.4231774	15047	.2200445	9439	.2565977	5608	-.0365532***

Table 1B Mean values for outcome variables for the whole population and the different subgroups

	(1) Whole population	(2) Female	(3) Male	(4) Urban	(5) Rural	(6) Female urban	(7) Female rural	(8) Male urban	(9) Male rural
	mean	mean	mean	mean	mean	mean	mean	mean	mean
Wealth score	21612.92	18906.18	25134.73	122152.5	-30122.89	123092.6	-33109.73	120983.9	-26143.99
Wealth index	3.099966	3.073649	3.134209	4.556837	2.350288	4.568966	2.3271	4.541759	2.381178
School years	7.749691	7.19734	9.042118	9.424716	6.903223	8.975039	6.334679	10.38843	8.295564
Secondary educ.	.5234005	.458263	.6757685	.746399	.4107447	.6959551	.3429288	.8545279	.576728
Worked past year	.6511731	.5664809	.8414784	.7291149	.6126441	.6432849	.5302582	.9037123	.8073744
Cash paid	.3969033	.3212539	.5649383	.5951322	.3003534	.4839498	.2451876	.8216063	.4283012
Self employed	.2336678	.2646463	.0538218	.2582337	.2222763	.2930368	.2511494	.0301109	.0632911
Work on own land	.490573	.4804499	.5131265	.5882353	.4860789	.6226415	.471891	.3076923	.5163636

4 Empirical strategy

I examine the causal relationship between education on the one hand and wealth and employment on the other. However, looking at the relationship using a simple OLS is likely to produce unreliable results. Estimates might be biased upward due to reverse causality, higher wealth leads to higher education level, or unobserved factors that affect both education and the outcomes of interest. In order to look at the causal effect, I will reduce these issues by taking advantage of the timing of the post-independence schooling reform (Keats, 2012). I will use the strategy of the regression discontinuity design and use whether or not an individual was above or below secondary school age when the reform was implemented as an instrument for education level. This will be discussed in detail in the following paragraphs.

I start with estimating the effect on secondary school attendance from the reform. I plot the conditional expectation of the observed outcome in Figure 1. There is a clear break in the trend between the cohorts that were 14 and 16 years of age in 1980, but we see no clear cut in the trend at the age of 15. Agüero and Bharadwaj (2012) get a clear jump at age 15 in 1980 by detrending in age. In the following analysis I will however use Figure 1, since it good to see the raw correlations. However, in all regressions I will control for age. The shape of the reform can explain the lack of a clear jump in school attendance at age 15 in 1980. In addition to making education more accessible, the reform removed the age restriction to allow over-age children to enter school. Individuals above 15 that gains access to secondary school because of the reform and choose to go back to school can explain the increasing trend in school attendance starting at age 16. The fact that the trend flattens out and stays at the same level for cohorts younger than 14 indicates that the change in trend is created by the reform.

If we saw a clear break at the age at 15 in 1980, the natural treatment group would be children who were younger then 15 in 1980 and the control group would be individuals who were older then 15. However in Figure 1, it looks like some of the individuals who are above 15 years of age in 1980 could have benefited from the reform as well. We can also see that the trend does not flatten out before for the cohorts younger than 14 years of age. Following Cannonier and Mocan (2012), I will therefore run all regressions with a sample including all cohorts and a second sample excluding the cohorts between 14 and 16 of age in 1980 from the analysis. In the second sample, the treatment group is individuals between 6-13 of age in 1980 and the control group 17-22 of age in 1980. Agüero and Bharadwaj (2012) using the same dataset and research design, drop the cohorts who are 14 and 15 years of age to get a clearer discontinuity.

Since the probability of attending secondary school jumps by less then 1 at the cutoff year, this is a fuzzy regression discontinuity (Imbens & Lemieux, 2008). Formally, in the fuzzy setting the causal effect having attended secondary school is defined as the difference in the outcome variable from a regression on the treatment-determining variable (age in 1980) divided by the difference in the treatment (attending secondary school) from a regression in the treatment-determining variable, both evaluated as the cutoff value, c (15 years of age in 1980) (Keats, 2012):

$$\tau_{FRD} = \frac{lim_{x \downarrow c} \mathrm{E}[Y|Age\ in\ 1980 = x] - lim_{x \uparrow c} \mathrm{E}[Y|Age\ in\ 1980 = x]}{lim_{x \downarrow c} \mathrm{E}[Educ|Age\ in\ 1980 = x] - lim_{x \uparrow c} \mathrm{E}[Educ|Age\ in\ 1980 = x]} \tag{1}$$

As noted in Van Der Klaauw (2002) we can estimate the effect at the discontinuity point, by using the age-cut off as an excluded instrument and apply an analogues two-stage regression approach. I estimate the effect of the reform on the probability of attending secondary school in the first stage:

$$Educ_i = \alpha_0 + \alpha_1 DumAge_i + \alpha_2 DumAge_i * (Age80 - 15) + \alpha_3 (1 - DumAge_i) * (Age80 - 15) + \alpha_4 x'_i + v_i \tag{2}$$

Where Educ takes the value of 1 if the individual *i* attended secondary school and 0 otherwise, the dummy variable DumAge takes the value of 1 if the individual *i* is younger then 15 years of age in 1980 and 0 otherwise. The two following terms controls for the trend in age on each side of the cut off. Following Agüero and Bharadwaj (2012), I use a linear approximation on either side of the cut off. The predicted value for secondary school attendance, $\widehat{Educ_i}$, is then used in the second stage:

$$Y_i = \beta_0 + \beta_{FRD} \widehat{Educ_i} + \beta_2 DumAge_i * (Age80 - 15) + \beta_3 (1 - DumAge_i) * (Age80 - 15) + \beta_4\, x'_i + \varepsilon_i \tag{3}$$

Where Y_i is the respective outcome variable and x'_i is a vector of controls including gender, marital status, survey round, and region. If the estimation strategy is valid, β_{FRD}, is the causal effect of secondary school on outcome Y. If the trend in age is controlled for correctly, β_{FRD} is numerically equivalent to τ_{FRD} from equation 1.

The validity of the regression discontinuity design rests on four assumptions. I will follow the setup and notation of Van Der Klaauw (2002) in the following explanation. The first assumption is that the individuals on either sides of the cut off would have similar average outcomes in absence of treatment. Formally, the outcome variable, both with and without treatment, is continuous as age crosses 15 years in 1980.

Assumption A1:

Both E$[Y(Educ = 1)|\ Age\ in\ 1980 = x]$ and $E[Y(Educ = 0)|\ Age\ in\ 1980 = x]$ are continuous in x.

If this holds we can write

$$lim_{x\downarrow 15}\ E[Y|\ Age\ in\ 1980 = x] - lim_{x\uparrow 15}\ E[Y|\ Age\ in\ 1980 = x] = \beta_{FRD} * (\ lim_{x\downarrow 15} E[Educ|\ Age\ in\ 1980 = x] - lim_{x\uparrow 15}\ E[Educ|\ Age\ in\ 1980 = x]) \quad (4)$$

We can see that if we solve this for the causal effect, β_{FRD}, we get expression (1). In my case assumption 1 indicates that for the causal effect to be valid the cohorts just above and just be-low 15 years of age in 1980 cannot differ in any other aspect then expected secondary schooling. One might argue that this is less likely to hold when the cohorts of 14, 15, and 16 years of age in 1980 are excluded from the regression. The difference in age is then larger between the cohorts in the control and treatment group, and there might unobservable differences between the groups due to the age difference. On the other hand, one might argue that those going back to school when the reform allows them access to the secondary education are different from the rest of the population in some aspects. I will argue for the validity of the first stage further in section 4.2.

For the second assumption for validity to hold, the treatment effect has to be continuous at the cut off age.

Assumption A2: The average treatment effect function E $[\beta_{FRD}|Age\ in\ 1980]$ is continuous as the age of 15 in 1980.

If the reform didn’t allow for the cohorts beyond starting age of secondary school to go back to school, the age of 15 in 1980 would completely determine the individuals who benefitted from the reform. The two previous assumptions would assure the validity of the estimated effect. However the reform opened up for older cohorts to go back to school. This might violate the third assumption that states that the choice of attending secondary school, conditional on the age in 1980, should be independent of return of education.

Assumption A3:

Educ_i is independent of β_{FRD_i} conditional on age in 1980 near the age of 15 in 1980

This is a strong assumption that might be violated by the cohorts that are allowed by the reform to go back to school and attend secondary school. If they self-select into secondary school conditional on their return to secondary school the assumption is violated. This would leave the cohorts just above the age of 15 to have higher average return to secondary school then the average of the population.

Finally the last assumption is analogous to the instrumental variable monotonicity assumption. The last assumption states that there should be no individuals who would have attended secondary school before the reform who wouldn't attend secondary school after the reform. If this assumption holds it leaves us with three groups of individuals; those who would attend secondary school both in the absence and in the presence of the reform, always-takers, those who would not attended secondary school neither in the absence nor in the presence of the reform, never-takers, and those who would not attend secondary school in the absence of the reform, but attends in the presence of the reform, compliers. It is this last group, the compliers, which the regression discontinuity design measures the average treatment effect for. The effect might be different for the two other groups. So it will be important to note who the compliers are, and I will discuss this further in section 5.3.5.

Further, it is also important to remember that the estimated effect is a local average treatment effect. In this case, that is the effect of secondary education for individuals just around the age of 15 in 1980. It is not certain that the effect is the same for other cohorts.

To account for the possibility of difference in labor market conditions between the surveys when looking at labor market outcomes, I will follow Keats (2012) and use survey fixed effect. I do this by making use of survey dummy variables. This gives me the variation between individuals with and without secondary education within each survey round, but leaves out the variation between the surveys. The problem with difference in the labor market condition is only a problem if there is an imbalance, between educated and not educated, in the people interviewed in any of the survey rounds. Keats (2012) also controls for condition in the labor market when individuals enter the labor market by the use of GDP growth. Since my time window is so small, I don’t see this as necessary.

The survey dummy variables are also important in respect to the wealth score. As stated in section 3, the wealth score is made relative to rest of the population in each survey round. The wealth index is therefore not comparable between survey rounds. By using survey dummy variables, I am left with only the difference in the wealth score between individuals with and without secondary education in the same survey. The wealth score is a household variable so marital status might have a big effect. I solve this by controlling for marital status in all the regression except for the once where martial status is the outcome variable.

Following Agüero and Bharadwaj (2012), I cluster the standard errors on an age-region level. The preferred level would be to cluster at the cohort level, but this leaves me with too few clusters, only 16 when the whole sample is used and 13 when the cohorts that that where from 14 to 16 years of age in 1980 are excluded. Clustering at the cohort-region level leaves me with, respectively, around 160 and 130 clusters. By clustering at cohort-district level I control for the possible inter cohort-region correlation.

4.1 Another estimation approach using the same identification

For all the variables, I will run both a linear regression and a linear instrumental variable regression. However, for the binary outcomes, such as if a respondent is employed or not, a nonlinear instrumental variable approach might be appropriate (Ozier, 2012). I will therefore also present the probit and the IV probit estimated effects for the binary outcomes. The IV probit uses the same first stage as in equation (2), but with a non-linear second stage:

$$\Pr[\text{employed} = 1] = \Phi\left(\gamma_0 + \gamma_{FRD}\widehat{Educ_\iota} + \gamma_2 DumAge_i * (Age80 - 15) + \gamma_3\left(1 - DumAge_i\right) * (Age80 - 15) + \gamma_4\, x'_i \right) \quad (5)$$

Where the variables are the same as in equations (3). The IV probit is only correctly specified if the first stage residuals are asymptotically normally distributed, and when the first stage is linear (Ozier, 2011). Here the first stage is binary as well as the second stage. However, we can see from table 2-4 that the coefficients from the linear first stage do not significantly differ from the non-linear probit estimations.

The results presented from the probit and IV probit estimations will be the average marginal effects. It is important to note that this is the average effect on individuals, and that this might differ from the effect on the average individual in non-linear models (Cameron & Trivedi, 2010).

4.2 Validity of the empirical strategy

As stated earlier, the validity of the design rests on the assumption that children just below 15 years of the age in 1980 are similar in unobservable ways to children just above the age of 15 in 1980. If other variables than education exhibit a discontinuity around the cutoff the RD approach is invalid. The effect on the outcome variable may then be caused by the discontinuity of another variable and I cannot say anything about the causal effect of education.

Agüero and Ramachandran (2012) look at the same reform with a different data set. They argue that a lack of discontinuity in the schooling level for the white Zimbabweans will make the validity argument stronger since the reform was targeted towards the black population. The DHS data does not contain information about the ethnicity of the respondent so I can't replicate this test. However, they find no discontinuity at the cut-off for white Zimbabweans.

Furthermore, Agüero and Ramachandran (2012) uses the Afrobarometer to see if the younger cohorts were more exposed to the new political and social environment that arose after the independence. The Afrobarometer is a set of nationally representative surveys that gather data on individual values and attitudes towards democracy, economic life, the quality of governance, engagement in civil society, and citizenship in several African countries. If the younger cohorts were more exposed to the new social environment the 2SLS estimator might be biased due to combined treatment. That the younger individuals would than be different in both education and other social environment. Agüero and Ramachandran (2012) find no difference in neither political involvement nor indicators of citizenship between the treatment and control group.

5 Results

5.1 First stage results

The first stage is shown graphically in figure 1 in the back of the paper. As stated in the previous sections, we can see a clear discontinuity in the average secondary school attendance for the population. The discontinuity in the trend for the population en masse is shown in panel A in figure 1. The outcome variable of the first stage regression, having attended secondary school, is binary. I have therefore done the estimation with both a linear OLS model and a non-linear probit model. The results are shown in table 2. Depending on controls, the OLS estimates a 15 percent increase in the probability of attending secondary school at the cut off of 14 in 1980. The non-linear estimation, the probit, for the same sample estimates a smaller effect of 13 percent points. However, the two estimates are not significantly different. I also find that the effect increases in magnitude when the cohorts in the in phasing period are excluded.

Table 2 - First stage with secondary education as outcome variable for the population as a whole, standard errors are clustered at the cohort region level

	(1) OLS	(2) Probit	(3) OLS	(4) Probit	(5) OLS	(6) Probit
Treatment dummy	0.331***	0.312***	0.160***	0.139***	0.151***	0.130***
- full sample	(0.0266)	(0.0221)	(0.0524)	(0.0500)	(0.0189)	(0.0171)
Observations	16767	16767	16767	16767	12454	12454
F-statistic	155.5		80.05		274.9	
Treatment dummy	0.385***	0.358***	0.266***	0.232***	0.242***	0.205***
- sample excluding the in pashing period	(0.0269)	(0.0215)	(0.0754)	(0.0756)	(0.0270)	(0.0248)
Observations	14258	14258	14258	14258	10637	10637
F-statistic	203.9		80.34		286.1	
Controlling for Age	NO	NO	YES	YES	YES	YES
Controlling for Marriage	NO	NO	NO	NO	YES	YES
Controlling for Survey year and Region	NO	NO	NO	NO	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Through out the paper I will look at the heterogonous effect of education in the population. I will start by looking at how the reform affected different groups differently. The effect on the secondary school attendance for the female and male population is shown in panels B and C in figure 1. For both male and female, we can see a clear change in the average secondary schooling for both groups. The jump is however less clear for the male population. However, when we control for age and other demographic variables, we can see from table 3 that the discontinuity is significant. Being younger than 15 in 1980 increases the probability of attending secondary school with 7-8 percent point. When we exclude the in phasing period the effect is stronger, but not significantly different. For the female population the discontinuity is clearer at the cut off point and significantly greater in magnitude. Controlling for age and other demographic variables, being younger the than 15 in 1980 increases the probability of having attended secondary school by 16-19 percent points. As expected the discontinuity increases when the in phasing period is excluded, however the increase is not significantly different from zero. From the graphs, we can also see that the expected secondary school attendance is higher for male than for female both before and after the reform was introduced.

Table 3 - First stage with secondary education as outcome variable for the subpopulations, standard errors are clustered at the cohort region level

	(1) Female OLS	(2) Female probit	(3) Male OLS	(4) Male Probit	(5) Urban OLS	(6) Urban Probit	(7) Rural OLS	(8) Rural Probit
Treatment dummy	0.193^{***}	0.159^{***}	0.0690^{**}	0.0796^{***}	0.116^{***}	0.109^{***}	0.172^{***}	0.137^{***}
- full sample	(0.0223)	(0.0218)	(0.0278)	(0.0268)	(0.0269)	(0.0225)	(0.0238)	(0.0221)
Observations	7619	7619	4835	4835	4178	4178	8276	8276
F-statistic	189.8		78.63		41.20		115.2	
Treatment dummy	0.267^{***}	0.224^{***}	0.176^{***}	0.171^{***}	0.190^{***}	0.156^{***}	0.272^{***}	0.224^{***}
- sample excluding the in phasing period	(0.0310)	(0.0320)	(0.0512)	(0.0452)	(0.0464)	(0.0330)	(0.0308)	(0.0329)
Observations	6450	6450	4187	4187	3569	3569	7068	7068
F-statistic	207.6		72.00		38.73		125.8	
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES
F-statistic	207.6		72.00		38.73		125.8	

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel D and E in figure 1 show the effect of the reform on expected secondary schooling for the urban and the rural population. For the urban population, we can see a clear change in secondary school attendance, but as for the male population the break in the trend is not so sharp. However, when controlling for age and other demographic controls, the discontinuity at the cut off is significant. Being younger then 15 in 1980, increases the probability of having attended secondary school by 11-12 percent points for the urban population. For the rural population the jump in expected probability is much sharper and larger in magnitude. Being younger than 15 in 1980 increases the probability for having attended secondary school by 14-17 percent points, depending on model specifications. As for the other subgroups, excluding the in phasing period enlarge the magnitude of the effect of being younger than 15.

Table 4 - First stage with secondary education as outcome variable for the subpopulations, standard errors are clustered at the cohort region level

	(1) Male rural OLS	(2) Male rural Probit	(3) Male urban OLS	(4) Male urban Probit	(5) Female rural OLS	(6) Female rural Probit	(7) Female urban OLS	(8) Female urban Probit
Treatment dummy - full sample	0.0889^{**}	0.0789^{**}	0.0289	0.0737^{**}	0.213^{***}	0.162^{***}	0.162^{***}	0.140^{***}
	(0.0372)	(0.0362)	(0.0393)	(0.0358)	(0.0275)	(0.0269)	(0.0364)	(0.0296)
Observations	3130	3130	1705	1705	5146	5146	2473	2473
F-statistic	38.88		8.781		63.09		23.44	
Treatment dummy - sample excluding the in pashing period	0.206^{***}	0.177^{***}	0.0976	0.0976	0.296^{***}	0.243^{***}	0.214^{***}	0.175^{***}
	(0.0617)	(0.0612)	(0.0894)	(0.0894)	(0.0356)	(0.0402)	(0.0643)	(0.0468)
Observations	2715	2715	1472	1472	4353	4353	2097	2097
F-statistic	39.64		8.156	8.156	68.12		22.39	
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In panel F and G of figure 1 the graphs for the rural and urban males are depicted. As I divide the male population into urban and male, the samples get small. The sample for the urban males is the smallest among the subgroups. This creates larger standard errors. For both the urban and the rural male population there is a lack of a clear jump in the graphs. However we see an increase in the average school attendance as the cohorts older than 14 in 1980 get younger and that this trends flattens out after the threshold. For the rural male populations the discontinuity is significant at a 5% significance level for the full sample and 1% significance level for the sample excluding the in phasing period. Being younger than 15 increases the probability of having attended secondary school with 8-20 percent points, depending on the sample. For the urban male population the discontinuity is not significant at any conventional significance level when using OLS. However, when using probit to estimate the effect of the reform, the magnitude of the estimate increases to 7 percent points and the standard errors decrease. The estimate is now significant at a 10% significance level. The difference between the two model specifications may be due to the fact that the expected outcome for secondary school attendance is as close to one for the treatment group (Ozier, 2011). I therefore choose to follow the probit estimate. However the instrument is weak for the male rural population. Staiger and Stock (1997) propose an F-statistic smaller then 10 as a rule of thumb for weak instruments. For the rural population the F-statistic is 8,16.

We see a difference in the effect from the reform on school attendance on rural and urban females. These results are shown graphically in panel H and I in table 4. For both subpopulations the jump in the probability of having attended secondary school is big and quite sharp. The rural female population is the subgroup that has the largest increase in the probability of secondary school attendance. Being younger then 15 years of age in 1980 increases the probability of having attended secondary school by 16- 21 percent points. As for the urban male population the difference between the OLS and the probit estimate is large for the rural female, however they are not significantly different. The large difference may be due to that OLS is less efficient of specification when the excepted outcome is close to zero as it is for the untreated rural females (Ozier, 2011). For the urban female population the reform leads to an increase of 14-16 percent points in the probability of having attended secondary school.

We have now seen that the effect of the reform is positive and significant for both the population as a whole and for each individual subgroup of the population, besides for the rural male population. The estimate for the rural male population is not robust to changes in sample or model. It is the only subgroup, where the F-statistic is lower then 10. For the rest of the samples the F-statistic is well above the rule of thumb for a weak instrument. The results also show a clear heterogeneity in the effect that the reform had on the different subgroups of the population. It is important to keep in mind that the differences in magnitude of the discontinuities do not mean that we should expect smaller or no effect for the groups that have smaller discontinuities. As stated in the previous section, the IV estimates the effect for the compliers. That is the effect for those who go from not attending secondary school to attending secondary school at the cutoff. It is this effect on those who were affected by the reform that I will turn to in the next subsection.

Table 5 Wealth index - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort region level

	(1) OLS - wealth index	(2) OLS - wealth score	(3) wealth index	(4) wealth score	(5) wealth index	(6) wealth score	(7) wealth index	(8) wealth score
Treated dummy - full sample	0.490***	31831.2***	0.303	27739.0	0.705	10842.4	0.470	14086.5
	(0.0235)	(1408.9)	(0.411)	(27331.8)	(1.547)	(115010.2)	(0.302)	(17582.9)
Treated dummy - in phasing period excluded	0.494***	32305.2***	0.313	32375.5	0.584	14862.6	0.369	22613.0
	(0.0265)	(1550.3)	(0.391)	(25695.6)	(1.440)	(103300.2)	(0.301)	(18218.6)
Observations	7608	7608	7608	7608	7608	7608	7608	7608
Controlling for Age	YES	YES	NO	NO	YES	YES	YES	YES
Controlling for Marriage	YES	YES	NO	NO	NO	NO	YES	YES
Controlling for Survey year and Region	YES	YES	NO	NO	NO	NO	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Education and wealth outcomes

To find the causal effect from education on wealth I will use both OLS and 2SLS estimations. For the 2SLS estimation I will use if an individual were younger or older than 15 in 1980 as instrument for the education level. The result for the population as a whole is reported in table 5. The OLS show a clear and positive correlation between having attended secondary school and current wealth. The result is significant both when including and excluding the in phasing period. When turning to the 2SLS estimates for education on wealth, I don’t find any effect. The estimated effect is positive both independent of controls and if the in phasing groups is included or not, but not significantly different from zero at any conventional significance level. However, the estimated effect from the 2SLS does not differ significantly from the OLS estimates. This suggests that schooling is perhaps exogenous, but it may also be that I do not have enough statistical power to detect differences between OLS and IV. Finally, it may also be that the local average treatment effect for my group of compliers is unrepresentative for the population. I will discuss this further in section 5.4. As there is no statistically significant difference, I choose to interpret the OLS coefficient causally as OLS is more efficient. This is surprising as it suggests that the selection into secondary schooling is exogenous. The lack in difference, leads me to trust the OLS for the causal effect. Following the OLS estimate, education leads to an increase in wealth equivalent to a third of a standard deviation from the mean. The magnitude of the effect is independent of the sample used.

Turning to the subgroups of the population, for which results are reported in table 6, signs of the effect and significance differ between the groups. For the female population the correlation between wealth and schooling, the OLS estimate, is significant and of the same magnitude as for the population en masse. However the estimated causal effect from secondary education is negative and significantly different from the OLS. In contrast to the population en masse, secondary schooling seems to be endogenous for the female population, and the OLS estimate is likely to be biased. The causal estimate, given from the 2SLS, is negative, but by less then one third of the standard errors and is not statistically significant. For the male population there is a positive correlation between wealth and secondary schooling. The estimated causal effect of education from the 2SLS does not differ significantly from the OLS estimate for either of the samples. I therefore choose to interpret the OLS estimate as causal. For the male population secondary schooling leads to an increase in wealth of the same magnitude as for the population en masse, one third of a standard deviation. For the urban population we see the same positive correlation between schooling and wealth. However, the causal estimate of education is negative and significantly different from the OLS estimate. The 2SLS estimates a decrease in wealth score equivalent to 22-41 % of a standard deviation from the mean, dependent on the sample used. However, neither of the estimates is statistically significantly different from zero. For the rural population both the correlation and the causal effect of education on wealth are positive. The causal estimate, from the 2SLS, is slightly bigger, but not significantly different from the OLS correlation. I therefor choose the OLS estimate that shows the same positive effect as for the population as a whole for both samples.

Table 7 shows the result from the regressions where the male and female populations are divided into rural and urban population. For the male rural population both the correlation and the causal effect of the education are positive. The estimated causal effect is much bigger in magnitude then the correlation, but due to large standard errors for the 2SLS estimate the

Table 6 Wealth index - IV with age in 1980 as instrument for secondary school, clustered standard errors at the cohort-region level

	(1) female OLS	(2) female 2SLS	(3) male OLS	(4) male 2SLS	(5) urban OLS	(6) urban 2SLS	(7) rural OLS	(8) rural 2SLS
Secondary school - full sample	37158.1***	-6241.7	34393.1***	112172.3	28917.0***	-20635.9	36592.6***	43100.8**
	(1926.8)	(19098.1)	(2536.0)	(102928.9)	(2396.7)	(24096.5)	(1929.8)	(21177.9)
Secondary school - in phasing period excluded	38245.3***	-5345.5	35188.7***	87615.2*	29640.6***	-39481.5	37015.5***	46280.2**
	(2128.6)	(20952.1)	(2865.8)	(48536.5)	(2601.0)	(30354.6)	(2161.7)	(18243.8)
Observations	4250	4250	3358	3358	2569	2569	5039	5039
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 7 Wealth index - IV with age in 1980 as instrument for secondary school, clustered standard errors at the cohort-region level

	(1) male rural OLS	(2) male rural 2SLS	(3) male urban OLS	(4) male urban 2SLS	(5) female rural OLS	(6) female rural 2SLS	(7) female urban OLS	(8) female urban 2SLS
Secondary school - full sample	33781.1***	189043.3	27595.2***	3482.3	38967.4***	18772.8	29462.1***	-33236.1
	(2911.1)	(171674.5)	(4158.0)	(144664.6)	(2427.5)	(21177.0)	(2775.7)	(30240.4)
Secondary school - in phasing period excluded	34232.9***	94963.3*	28027.4***	105605.0	39138.7***	31258.4*	30258.4***	-113925.8*
	(3359.1)	(56790.6)	(4487.9)	(96101.4)	(2711.9)	(17136.7)	(2862.0)	(69152.0)
Observations	2190	2190	1168	1168	2849	2849	1401	1401
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

estimate is not significantly different from OLS estimate. I therefore choose the OLS estimate as it is more efficient. The estimated effect of education on wealth is then of the same magnitude from the rural male population as for the population as a whole and is robust to changes in the sample used. Also for the urban male population both the correlation and the causal effect are positive. Whether the causal effect is larger or smaller in magnitude than the estimated correlation depends on the sample used. However, also here the 2SLS estimated standard errors are large and the causal effect does not significantly differ from the OLS estimate. The estimated increase in wealth for the rural male population is not significantly different then from the population as a whole. However, it is important to remember that age in 1980 is a weak instrument for secondary schooling for the rural male population and might be biased. For the rural female population both the correlation and the causal effect are positive. The estimated causal effect is smaller then the correlation for both of the samples, but the estimates from the 2SLS does not differ significantly from the OLS estimates. The estimated effect of education is significantly higher for the rural female population than for the urban male population and the population en masse. However, it is not significantly different from the rural male population. For the female urban population the correlation between wealth and schooling is positive, but the estimated casual effect is negative. The estimates from the 2SLS regression differ significantly from the OLS estimates. The magnitude and the significance of the estimated causal effect depend on the sample used. When the in phasing period is excluded, having attended secondary school reduces the wealth score by more then one standard deviation form the mean. However, the result is only significant at a 10% significance level.

For the population as a whole, I find a positive effect of secondary schooling on wealth equivalent to be one third of a standard deviation. This is in line with Duflo (2001) and Ozier (2011) who find higher wages due to education. In the lack of a significant difference between the estimates given from the OLS and the 2SLS, I treat secondary education as exogenous for the population as a whole and for some of the subgroups. For the male and rural male I find a positive and significant effect that does not differ significantly from the effect on the population en masse. For both the rural and rural female population the effect is positive and significantly higher than for the population as a whole. I do not find a significant causal effect of education on wealth for the female and the urban population. The lack of a positive effect on the female population is in contrast to Keats (2012), who finds a positive effect due to education when looking at the same wealth score in Uganda. As the only subgroups, the urban female population has a negative effect of education on wealth. However, the result is only significant at a 10% significance level and only when the in phasing period is excluded. In the next section I will look for possible mechanism behind these results.

5.3 Mechanisms

In the following, I will look at possible mechanisms behind the effect education has on wealth. One possible mechanism behind the difference in effects is a difference in compliers in the different subgroups. The reform provides exogenous variation in school attendance. And by taking advantage of this variation by using regression discontinuity design, we are ensured that the difference in school attendance between the cohorts is not because of selection into schooling. However, one might think that more people attended secondary school in urban than in rural areas pre-reform and more males than females. This might cause the population who attended secondary school due to the reform, the compliers, to be different between urban and rural, and female and male. Another possible mechanism is that other variables that are positive for wealth accumulation are affected by the having attended secondary school. The regression discontinuity approach, if valid, ensures that the only variable that is different between the control and treatment group is the expected education level at the point of time when the decision of school attendance is taken. This ensures that any effect that we see on wealth is due to difference in secondary school attendance. However, the approach does not ensure that the effect goes directly from having attended secondary school to being wealthier. It might be that having attended secondary school affects some other variable that again affects the wealth. I will next begin looking at how education affects other variables that might be correlated with wealth and then turn to looking at possible differences in compliers between the subgroups.

5.3.1 Employment

Employment is one variable that might be affected by education and might effect wealth accumulation. I will start out by examining whether or not education affects the probability of being employed and then turn to if different levels of educations lead to different types of employment.

Table 8 The correlation between employment variables and wealth, standard errors clustered on cohort-region level

	(1) Worked past year	(2) Cash paid	(3) Working on own land	(4) Self employed
Correlation with the wealth score	17116.2***	28720.9***	-22201.2***	-3706.1**
	(1606.2)	(1818.1)	(4133.0)	(1857.4)
Observations	8932	8922	1070	5042
Controlling for Age	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Employed

Here I use whether or not an individual has worked the past year as measurement for employment. There is a clear positive correlation between having worked in the past year and wealth. Having worked in the past year increases the wealth score with almost a fifth of a standard deviation from the mean. This correlation and the correlation for the other possible mechanism variables are reported in table 8. If I find an increase in employment from secondary schooling for the same groups that we saw an effect on wealth, it might be that having attended secondary school affects wealth thorough employment.

Table 9 Worked past year - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) OLS	(2) Probit	(3) 2SLS	(4) IVprobit	(5) 2SLS	(6) IVprobit	(7) 2SLS	(8) IVprobit
Secondary school - full sample	0.0945***	0.0895***	-0.00336	-0.00818	0.243	0.227	0.0759	0.0623
	(0.00917)	(0.00854)	(0.0731)	(0.0729)	(0.284)	(0.255)	(0.0991)	(0.0986)
Secondary school - in phasing period excluded	0.0920***	0.0870***	-0.000962	-0.00380	0.315	0.289	0.135	0.103
	(0.0102)	(0.00936)	(0.0734)	(0.0734)	(0.427)	(0.327)	(0.151)	(0.143)
Observations	9735	9735	13347	13347	13347	13347	9735	9735
Controlling for Age	YES	YES	NO	NO	YES	YES	YES	YES
Controlling for Marriage	YES	YES	NO	NO	NO	NO	YES	YES
Controlling for Survey year and Region	YES	YES	NO	NO	NO	NO	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Since the outcome variable is binary, I will run all regressions both as a linear 2SLS model and an IV probit. The results are reported in table 9. For the population as a whole there is a positive and significant correlation between having attended secondary school and having worked during the last year. The causal estimates, from the 2SLS and IV probit, are slightly smaller, but do not differ significantly from the OLS estimate. This leads me to trust the OLS estimates for the causal effect. Both models show that individuals who have attended secondary school are 9 percent points more likely to be employed.

Table10 Worked past year - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) female OLS	(2) female 2SLS	(3) female Probit	(4) female IVP	(5) male OLS	(6) male 2SLS	(7) male Probit	(8) male IVP
Secondary school - full sample	0.119^{***}	0.212^{*}	0.117^{***}	0.206^{**}	0.0399^{***}	-0.672	0.0334^{***}	-0.466
	(0.0126)	(0.113)	(0.0122)	(0.103)	(0.0137)	(0.801)	(0.0118)	(0.378)
Secondary school - in phasing period excluded	0.113^{***}	0.0972	0.112^{***}	0.0971	0.0367^{**}	-0.460^{*}	0.0292^{**}	-0.378^{**}
	(0.0142)	(0.130)	(0.0137)	(0.129)	(0.0146)	(0.280)	(0.0126)	(0.189)
Observations	6449	6449	6449	6449	3358	3358	3358	3358
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Looking at the effect on the male and female population separately, I find heterogeneous effects. The results are reported in table 10. For the female population both the correlation and the causal effect of education are positive independent of sample and model used. Whether the causal effect is smaller or larger in magnitude than the correlation depends on the sample used. However the 2SLS and IV probit are never significantly different from the OLS and probit estimates. I therefor take secondary schooling as exogenous and follow the OLS and probit estimates. The effect of education is then an11-12 percent point increase in the probability of being employed, depending on the sample used. For the male population, the correlation is positive while the estimated the causal effect are negative. I see no significant difference between the casual estimates and the OLS and probit estimates besides for the non-linear estimation for the sample excluding the in phasing period. Following the

OLS and probit model for the remaining samples, we see a 3-4 percent point increase in the probability of being employed. For the sample excluding the in phasing period, the IV probit show a 39 percent point increase in the probability of being employed. The estimated effect on being employed of education is significantly smaller for men than for women, if we exclude the IV probit estimation from the sample excluding the in phasing period for males.

Table 11 Worked past year - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) urban OLS	(2) urban 2SLS	(3) urban Probit	(4) urban IVP	(5) rural OLS	(6) rural 2SLS	(7) rural Probit	(8) rural male IVP
Secondary school - full sample	0.0949*** (0.0173)	-0.290 (0.188)	0.0832*** (0.0149)	-0.274* (0.144)	0.0958*** (0.0115)	0.189 (0.120)	0.0949*** (0.0111)	0.178 (0.112)
Secondary school - in phasing period excluded	0.0825*** (0.0184)	-0.258 (0.229)	0.0729*** (0.0160)	-0.279 (0.184)	0.0934*** (0.0125)	-0.00693 (0.118)	0.0925*** (0.0119)	-0.0128 (0.116)
Observations	3264	3264	3264	3264	6543	6543	6543	6543
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The estimated effects on the urban and rural population are depicted in table 11. The correlation and the causality for the urban population differ in signs. For both samples, the correlation between education and wealth is positive while the causal effect is negative. Looking at the sample including all cohorts, there is a significant difference between the causal estimates and the OLS and probit. The causal effect is estimated to a 27-29 percent point decrease in the probability of being employed, depending on model. It is, however, only the IV probit estimate that is significant and then only at a 10 % significance level. For the sample excluding the in phasing period, there is no significant difference between the causal estimates and the OLS and probit estimates. Following the OLS and probit estimates, I find a 7-8 percent point increase in the probability of being employed. For the rural population there is no significant difference between the causal effects and the OLS and probit estimates.

Following the OLS and probit estimates, we see a 9-10 percent point increase in the probability of being employed for the rural population.

Table 12 Worked past year - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) male rural OLS	(2) male rural 2SLS	(3) male rural Probit	(4) male rural IVP	(5) male urban OLS	(6) male urban 2SLS	(7) male urban Probit	(8) male urban IVP
Secondary school - full sample	0.0437*** (0.0159)	-1.012 (1.221)	0.0414*** (0.0150)	-0.538*** (0.166)	0.0165 (0.0276)	-0.00285 (0.757)	0.0145 (0.0213)	0.126 (0.784)
Secondary school - in phasing period excluded	0.0371** (0.0170)	-0.389 (0.299)	0.0337** (0.0159)	-0.337* (0.199)	0.0242 (0.0302)	-1.069 (1.199)	0.0202 (0.0227)	-0.816* (0.432)
Observations	2190	2190	2190	2190	1168	1168	1168	1168
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The estimated effects for the male and female population, when divided into rural and urban, are present in table 12. For the rural male population, the correlation estimates are positive and the causal estimates are negative. There is no significant difference between the causal effects and the OLS and probit estimates for any of the models when the sample excluding the in phasing period is used or for the linear estimation for the sample including all cohorts. Following the OLS and probit for these specification, gives an estimated effect on the probability of being employed by 3-4 percent points. For the non-linear estimation using the full sample, the probit and IV probit gives significantly different coefficients. The causal effect given by the IV probit is a 53 percent point decrease in the probability of being employed and is significant at a 1% significance level. For the urban male population the correlation is positive independent of sample and model while the directions of the causal effects vary. However, only the IV probit estimation for the sample excluding the in phasing period is statistically significant and then only at a 10 % significance level. But this estimate, like the rest of the causal estimates, is not significantly different from the probit result for the same sample. So following the OLS and probit estimates for the causal effect I find no effect from education on the probability of being employed for the urban male population.

Table 13 Worked past year - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) female rural OLS	(2) female rural 2SLS	(3) female rural Probit	(4) female rural IVP	(5) female urban OLS	(6) female urban 2SLS	(7) female urban Probit	(8) female urban IVP
Secondary school - full sample	0.118^{***} (0.0146)	0.365^{***} (0.130)	0.118^{***} (0.0143)	0.332^{***} (0.0901)	0.117^{***} (0.0218)	-0.262 (0.204)	0.114^{***} (0.0206)	-0.247 (0.160)
Secondary school - in phasing period excluded	0.116^{***} (0.0165)	0.149 (0.155)	0.116^{***} (0.0161)	0.152 (0.147)	0.101^{***} (0.0234)	-0.0965 (0.246)	0.0978^{***} (0.0223)	-0.111 (0.236)
Observations	4353	4353	4353	4353	2096	2096	2096	2096
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The effects for the female rural and urban population are found in table 13. For the rural female population both the correlation and the causal effect are positive for all specifications of both sample and model. Only for the non-linear model for the sample including all cohorts does the correlation estimate and causal effect estimate differ significantly. For this sample the IV probit estimates a 33 percent increase in the probability of being employed due to secondary education and the estimate is significant at a 1% significance level. For the remaining sample and model specifications, following the OLS and probit estimates gives an estimated effect of 12 percent points increase in the probability of being employed. For the urban female population the correlation is positive and the causal effect is negative independent of model and sample used. It is only for the non-linear specification for the full sample, that the causal effect is significantly different from the correlation coefficient. The IV probit for this specification estimates a 24 percent point degrees in the probability of being employed from secondary schooling. However, the estimate is no significantly different from zero at any conventional significance level. For the remaining specification, the OLS and probit estimates a 10-12 percent point increase in the probability of being employed.

For the population as a whole we see a positive effect of education on the probability of being employed. The effect is also positive for all the subgroups, besides from the urban, urban male, and the urban female population. The effect for the urban population is not robust to changes in sample or model, and the only significant result shows a negative effect. For the urban male population I find no effect. The estimate for the urban female population is not robust to changes in the sample or specification. The positive effect found for the female population is significantly larger than that for the male population. I will no go on to looking at how education affects the probability of different types of employment.

Getting paid in cash

Also between getting paid in cash and wealth, there is a positive correlation. Individuals who get paid in cash have a one third of a standard deviation from the mean higher wealth.

Table 14 Being paid in cash - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) OLS	(2) Probit	(3) 2SLS	(4) IVprobit	(5) 2SLS	(6) IVprobit	(7) 2SLS	(8) IVprobit
Secondary school - full sample	0.115^{***}	0.111^{***}	0.105^{*}	0.103	0.231	0.233	0.114	0.123
	(0.0106)	(0.0101)	(0.0631)	(0.0654)	(0.234)	(0.218)	(0.0978)	(0.0974)
Secondary school - in phasing period excluded	0.111^{***}	0.108^{***}	0.101	0.0977	0.127	0.129	0.0971	0.0948
	(0.0116)	(0.0112)	(0.0613)	(0.0639)	(0.354)	(0.367)	(0.136)	(0.136)
Observations	9725	9725	11061	11061	11061	11061	9725	9725
Controlling for Age	YES	YES	NO	NO	YES	YES	YES	YES
Controlling for Marriage	YES	YES	NO	NO	NO	NO	YES	YES
Controlling for Survey year and Region	YES	YES	NO	NO	NO	NO	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The effect of education on getting paid in cash for the population en masse, is reported in table 14. The correlation estimates are positive, independent of controls, model, and sample specifications. For neither model specification, the correlation estimate differs from the estimated causal effects. Therefore, following the OLS and probit estimates, I find 11-12 percent points increase in the probability of getting paid in cash from education for the population en masse.

Table 15 Being paid in cash - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) female OLS	(2) female 2SLS	(3) female Probit	(4) female IVP	(5) male OLS	(6) male 2SLS	(7) male Probit	(8) male IVP
Secondary school - full sample	0.132^{***}	0.215^{**}	0.130^{***}	0.214^{**}	0.0884^{***}	-0.547	0.0792^{***}	-0.392
	(0.0131)	(0.0927)	(0.0127)	(0.0862)	(0.0182)	(0.764)	(0.0166)	(0.369)
Secondary school - in phasing period excluded	0.123^{***}	0.118	0.123^{***}	0.118	0.0736^{***}	-0.807^{*}	0.0654^{***}	-0.506^{***}
	(0.0146)	(0.127)	(0.0143)	(0.128)	(0.0199)	(0.484)	(0.0182)	(0.129)
Observations	6446	6446	6446	6446	3350	3350	3350	3350
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results for the male and female population are reported in table 15. For the female population, I find both positive correlation and causal effects. The estimates for the causal effect do not differ significantly from the OLS and probit estimates for any of the specifications. Following the OLS and probit estimates, I find a positive effect of 12-13 percent point increase in the probability of getting paid in cash from education. For the male population the OLS and probit show a positive correlation while the 2SLS and IV probit show a negative causal effect. However, the estimates are not significantly different, besides for the non-linear specification of the sample excluding the in phasing period. For this specification, the estimated effect is a 51 percent point decrease in the probability of being employed and the estimate is significant. For the rest of the specification, following the OLS and probit, the causal effect estimates a 7-9 percent point increase in the probability of being getting paid in cash.

Table 16 Being paid in cash - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) urban OLS	(2) urban 2SLS	(3) urban Probit	(4) urban IVP	(5) rural OLS	(6) rural 2SLS	(7) rural Probit	(8) rural male IVP
Secondary school - full sample	0.120^{***}	-0.236	0.108^{***}	-0.237	0.119^{***}	0.210^{*}	0.117^{***}	0.220^{**}
	(0.0207)	(0.194)	(0.0185)	(0.156)	(0.0132)	(0.122)	(0.0130)	(0.109)
Secondary school - in phasing period excluded	0.100^{***}	-0.447	0.0902^{***}	-0.390^{**}	0.111^{***}	0.0197	0.110^{***}	0.0240
	(0.0225)	(0.301)	(0.0204)	(0.180)	(0.0144)	(0.140)	(0.0143)	(0.141)
Observations	3262	3262	3262	3262	6534	6534	6534	6534
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The result for the rural and urban population is presented in table 16. Independent of model specification and sample used, the correlation is positive and the estimates for the causal effect are negative for the urban population. For the linear specification, the correlation and causal effect estimate does not differ significantly. Following the OLS for the causal effect gives a 10-12 percent point increase in the probability of being paid in cash. For the non-linear specification the causal effect and the correlation differs significantly due to smaller standard errors. The estimated effect for this specification is a 24-40 percent point decrease. However, the estimate is only statistically significant when the in phasing period is excluded and then at a 5% significance level. For the rural population both the correlation and causal effect estimates are positive, independent of specifications. The estimates from the correlation and the causal effect do not differ significantly. Following the OLS and probit for the causal effect, I find 11-12 percent point increase in the probability of being paid in cash from education.

In table 17 the results from the male population, divided in to rural and urban, are presented. For the rural male population the correlation estimates are positive, while the estimates for the causal effect are negative. For the linear estimation there is no significant difference between the correlation and causal effect estimate. Following the OLS and probit estimates I find an 8-10 percent points increase in the probability of being paid in cash from

Table 17 Being paid in cash - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) male rural OLS	(2) male rural 2SLS	(3) male rural Probit	(4) male rural IVP	(5) male urban OLS	(6) male urban 2SLS	(7) male urban Probit	(8) male urban IVP
Secondary school - full sample	0.103***	-0.789	0.103***	-0.465**	0.0262	-0.137	0.0258	0.0498
	(0.0214)	(1.083)	(0.0213)	(0.229)	(0.0342)	(1.080)	(0.0286)	(1.056)
Secondary school - in phasing period excluded	0.0841***	-0.774	0.0844***	-0.465***	0.0283	-1.336	0.0267	-0.849**
	(0.0237)	(0.576)	(0.0236)	(0.133)	(0.0374)	(1.446)	(0.0316)	(0.350)
Observations	2183	2183	2183	2183	1167	1167	1167	1167
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

education. Due to smaller standard errors, the correlation and causal effect estimates differ for the non-linear estimation. The IV probit estimates a 47 percent point, independent of sample, decrease in the probability of being employed from education and the estimate is significant at a 1% significance level for both samples. For the urban male population only the IV probit for the sample excluding the in phasing period is significant. For the remaining samples the estimated causal effect does not differ from the estimated correlations effects. The estimated correlations are all positive, but not significantly different from zero. For the non-linear estimation, the estimated causal effect differs from the correlation and is significantly different from zero at a 10 % significance level. For this specification, the estimated effect from education is an 85 percent point decrease in the probability of being paid in cash. Again it is important to keep in mind that the age in 1980 is a weak instrument for education level for the urban male population.

The estimated effects for the female population, divided in rural and urban, are depicted in table 18. Both estimated correlation and causal effect are positive for the rural female population. Only for the non-linear specification of the sample including all cohorts, does the estimated correlation and causal effect differ. For this specification, the IV probit estimates a 31 percent points increase in the probability of being paid in cash from education and the estimate is significant at a 1 % significance level. For the remaining specifications,

Table 18 Being paid in cash - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) female rural OLS	(2) female rural 2SLS	(3) female rural Probit	(4) female rural IVP	(5) female urban OLS	(6) female urban 2SLS	(7) female urban Probit	(8) female urban IVP
Secondary school -full sample	0.125***	0.337***	0.123***	0.312***	0.145***	-0.231	0.142***	-0.216
	(0.0146)	(0.118)	(0.0141)	(0.0874)	(0.0262)	(0.214)	(0.0252)	(0.173)
Secondary school - in phasing period excluded	0.121***	0.220	0.120***	0.210	0.122***	-0.344	0.120***	-0.297
	(0.0163)	(0.151)	(0.0159)	(0.138)	(0.0283)	(0.338)	(0.0274)	(0.236)
Observations	4351	4351	4351	4351	2095	2095	2095	2095
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

following OLS and probit estimates, the estimated effect of education is 12-13 percent increase in the probability of being employed. For the urban female population the correlation estimates are positive and the estimated causal effects are negative. However, only for the non-linear model for the sample including all cohorts the estimates differ significantly. For the remaining samples the causal effect, following the OLS and probit estimates, is an estimated 12-15% increase in the probability of being paid in cash. For the sample including all cohorts the IV probit estimates a 22 % decrease in the probability of being paid in cash. However, the result is not significantly different from zero at any conventional significance level.

For the population as a whole, I find a positive effect of education on the probability of being paid in cash. The effect is robust and positive for the female, rural and rural female population. This is inline with Keats (2012), who finds a positive effect on employment for women in Uganda. The sign of the estimate for the urban population is not robust to changes in the sample or model. For the urban male population the estimated effect is only significant for the non-linear specification for the sample excluding the in phasing period, and is then negative. The estimate for the rural female population is not robust to differences in the specification of model or samples either. For the rural and rural male population the direction of the effects is dependent on the model specification.

Working on own land

I will here compare those who work on their own land to the rest of the population who works in the agriculture sector. Since the variable is for the agriculture sector, I will focus on the rural population for this variable. The rural population who work on own land have close to on third of a standard deviation from the mean less wealth then the rest of the population who work in the agriculture sector.

In table 19 the results for the population who work for agriculture sector are presented. There is a significant and positive correlation between working on your own land and having attended secondary school. When looking at the causal effect, the direction of the effect depends on the controls. However, it is only for the non-linear specification for the sample excluding the in phasing period that the correlation estimate differs from the estimates for the causal effect. For this specification the IV probit estimates a 40 percent point increase in the probability of working on own land from education and the estimate is significant at a 1 % significance level. For the remaining specification, following the OLS and the probit, the effect is estimated to 6-7 percent point increase.

Table 19 Working on own land - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) OLS	(2) Probit	(3) 2SLS	(4) IVprobit	(5) 2SLS	(6) IVprobit	(7) 2SLS	(8) IVprobit
Secondary school - full sample	0.0633^{***}	0.0635^{***}	-0.130	-0.125	0.213	0.207	0.152	0.170
	(0.0237)	(0.0237)	(0.0918)	(0.0824)	(0.315)	(0.288)	(0.240)	(0.228)
Secondary school - in phasing period excluded	0.0746^{***}	0.0753^{***}	-0.139	-0.133^{*}	0.300	0.282	0.427^{*}	0.397^{***}
	(0.0254)	(0.0251)	(0.0904)	(0.0800)	(0.343)	(0.276)	(0.233)	(0.133)
Observations	1401	1401	2216	2216	2216	2216	1401	1401
Controlling for Age	YES	YES	NO	NO	YES	YES	YES	YES
Controlling for Marriage	YES	YES	NO	NO	NO	NO	YES	YES
Controlling for Survey year and Region	YES	YES	NO	NO	NO	NO	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Looking at the male and female population separately, I find the same positive effect. These results are shown in table 20. For the male population the estimated causal effect only differs from the estimated correlation for the non-linear specification for the sample excluding the in phasing period. For this specification, the IV probit estimates a 47 percent point increase in the probability of working on own land due to education. The estimate is significant at a 1 % significance level. For the remaining specification, following the OLS and probit estimates, I find 7-9 percent point increase. However, the estimates are only significant at a 10 % significance level. For the female population, the causal effect estimates don't differ from the estimated correlation for any of the specifications. Following the OLS and probit estimates, I find a 14-17 percent increase in the probability of working on own land.

Table 20 Working on own land - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) male rural OLS	(2) male rural 2SLS	(3) male rural Probit	(4) male rural IVP	(5) female rural OLS	(6) female rural 2SLS	(7) female rural Probit	(8) female rural IVP
Secondary school - full sample	0.0728^{*}	0.625	0.0700^{*}	0.463^{*}	0.147^{***}	0.122	0.142^{***}	0.125
	(0.0425)	(0.815)	(0.0407)	(0.240)	(0.0336)	(0.228)	(0.0314)	(0.227)
Secondary school - in phasing period excluded	0.0865^{*}	0.610	0.0838^{*}	0.469^{***}	0.167^{***}	0.432	0.161^{***}	0.391^{**}
	(0.0455)	(0.449)	(0.0430)	(0.136)	(0.0343)	(0.290)	(0.0310)	(0.187)
Observations	511	511	511	511	890	890	890	890
Controlling for Age	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I find a positive effect of education on the probability of being working on own land for the rural population en bloc, the female, and the male population. This is contrary to what I expected to find. We saw in table 6 that the effect of education on wealth is positive for these groups. However, the correlation between wealth and working on own land and wealth is negative. It is important to keep in mind that the working own land regression only takes into account the part of the population that is working in the agriculture sector.

Self employed

Being self employed is correlated with lower wealth than other types of employment. As we can see from table 1A, a very small portion, only 5 %, of the male population is self employed. I will there for focus on the female population when looking at the effect of education on being self employed. About 26 % of the female population is self employed. These women have 4 percent of a standard deviation from the mean lower wealth then the rest of the population.

After having looked at both the population en masse and the different subpopulation, I can not find any significant causal effect between secondary education and being self employed. The results are reported in table A1-A2 in the appendix. The correlation between having attended secondary school and being self employed also lacks statistically significance. However, when not controlling for age and demographic factor, the causal effect is negative and highly significant. But as stated, this effect is not robust to controlling for age or demographic controls. For rural female the effect of education is positive and for urban female the effect is negative. But for neither subgroup the effect is statically different from zero at any conventional significance level. This is in contrast to Keats (2012), who finds a reduction in self-employment due to education for women in Uganda. That Keats (2012) studies the effect conditional on working, while I use the entire female population as reference groups, might in part explain the difference.

5.3.2 Difference in compliers

In the following I will explore whether the differences in effects can be caused by differences in the compliers in the different subgroups. As stated earlier, the magnitude of the effect from the reform on expected schooling doesn't effect the estimated outcome, but who gets affected by the reform might. It is therefore interesting to look at the possible difference in compliers between the subpopulations. This is hard to test, since the data set does not include data on the parents or the household the individuals grew up in. However, looking back at Figure 1 we can see how the school attendance differed between the different subgroups both before and after the reform.

Both for wealth and the probability of being employed, we find a stronger effect on the rural population then for the population en masse. Looking back at figure 1, we can see that the average secondary schooling for these groups is lower then for the population as a whole and the rest of the groups. Realizing that there is other factors that effect wealth and employment, and that these effects might be correlated with the selection of attending secondary school, might help us explain the result. If it so that the individuals who have the other factor for accumulation wealth and accumulation, are the first in their subpopulation to acquire secondary education, the compliers in the different subpopulation might differ in the other factors. It may be that there are other constraints that prevent the individuals who are the last in their subgroups to obtain secondary education from wealth and employment then education. For the rural population, we see that the pre- and post-reform attendance is lower than for the population en masse. The compliers in this group might be better equipped in other aspects than the compliers for the population as a whole and the other sub-groups. This might explain the stronger effect for this group.

This possible explanation conflicts with the result I find for the female population. For the female population I find no effect on wealth of education. The estimates for the causal effects are negative, but not significantly different from zero. However, the average school attendance post- and pre-reform is similar to that of the rural population.

Finally, differences in significant effects might also be due to sample sizes. I find more significant results for the female and rural population compared to the male and urban population. The samples for the two former populations are about twice the size of the male and rural population. This leads generally smaller standard errors for the rural and female population compared to the urban and male populations.

5.4 Education as exogenous

For both the population as a whole and many of the subpopulations, I find no significant difference between the OLS and the IV estimates when looking at wealth. This is likely to be due to several factors. First of all, the IV estimates are less efficient and in many cases the differences are quite large but I fail to reject them due to even larger standard errors. Therefore it may be that I do not have enough statistical power to be sure that there really is not any difference between IV and OLS. Another reason may be due to the complier group. In any case, I have chosen to treat the education level as exogenous when looking at the effect on wealth. This may seem unlikely and the caveats above should be kept in mind.

Duflo (2001) finds the same lack in significant difference in between the OLS and 2SLS estimates. She argues that this is in contrast to the view that the OLS estimates are biased upwards as a result of omitted family and community background variables. It is important to note here that Duflo (2001)'s IV results are significant and all of the same direction as the OLS estimates.

6 Conclusion

This paper has examined the economic returns to education and the heterogeneous effect between subpopulations. Taking advantage of a reform in 1980 that increased the attendance in secondary school significantly, I have used the age in 1980 as an instrument for education to look at the causal effect. I find the instrument to be strong for the population en masse and all subpopulations expect for the urban male population. For the population en masse I find a positive effect of education for the population as a whole. This effect is in line with the finding of Duflo (2001) from Indonesia and Ozier (2011) from Kenya, who find education leads to higher wages. Looking at the heterogeneous effects among different subgroups of the population, I find that the effect of education on wealth is significantly stronger for the rural and the rural female population then for the population en masse. For the female population as a whole I do not find a significant increase in wealth due to education. This is in contrast to Keats (2012)'s findings from Uganda that show an increase in women's wealth due to education. The lack in a significant effect from education for the female population might be driven from the suggested negative effect of education on wealth for urban women.

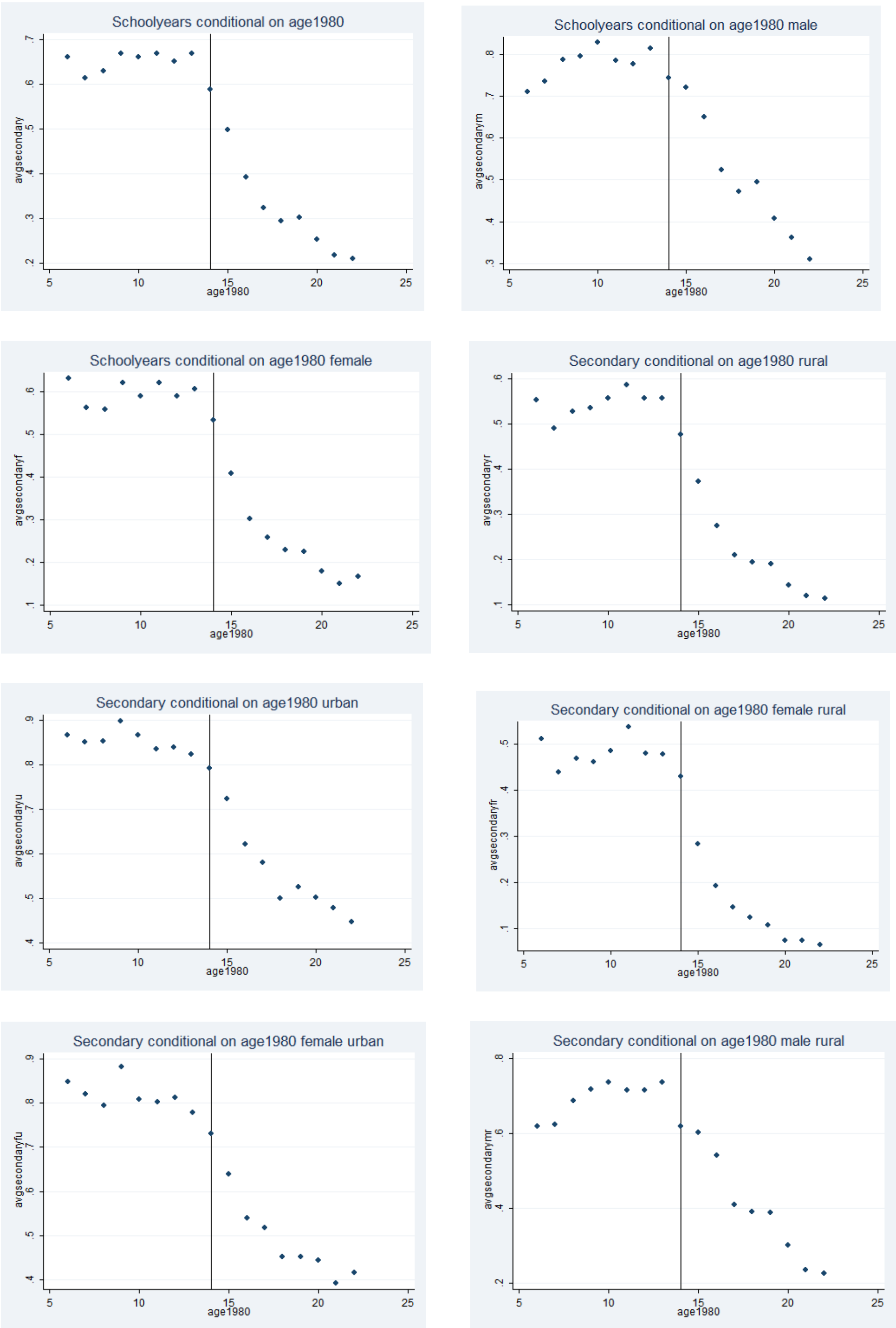
Looking for possible mechanism behind the findings, I find positive employment effects due to education. For the probability of being employed I find a positive effect for the population as a whole, the rural, the female and the male population. The effect is stronger for female than for males, and the effect is significantly stronger for the rural female population. For the remaining subpopulation I don't find any effects. This might be a part of the explanation for the stronger effect on wealth I find on rural and female rural population. When looking at the difference in type of employment, I again find stronger effects on the probability of getting paid in cash for the female, the rural female and the rural populations. Contrary to what one might expect, I find that education increases the probability of working on own land conditional on working in the agriculture sector, witch is negatively correlated with wealth. This suggest that the positive effect education has on wealth in the rural population is due to those who are not working in the agriculture sector. In contrast to Keats (2012) I find no effect of education on the probability of being self-employed for women.

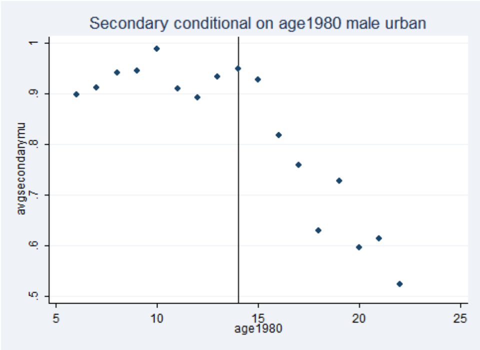
It is important to note that I for wealth did not find statistically significant differences between the 2SLS and OLS estimates, and therefore chose the OLS estimates as causal estimator. However, in many cases the differences were quiet large, but I fail to reject them due to larger standard errors for the IV estimates. Duflo (2001) also fails to reject the difference between the 2SLS and OLS estimates, but her differences are much smaller.

My main result is in line with previous research done on the economic return to education in developing countries (Duflo, 2001; Keats, 2012; Ozier, 2011). However, the validity of the result is both dependent on time, compliers and education level. The effect found is for individuals who entered secondary school in the 1980's on their wealth level in 2005 and 2010. The effect might very well be different for cohorts obtaining their education today. The effect found is also specific for the individuals who entered school due to the reform in 1980. The reform was targeting the black population of Zimbabwe. They might have constraints that affect their economic return differently than other groups. These constraints might also change over time. The general education level of education might also affect the result. For the subpopulation with the highest attendance both post- and pre-reform, the urban population, I fail to find any effect of education on wealth. This suggests that the return to education might be decreasing in the number of people obtaining education. It is also important to note that the effect found is the effect from secondary schooling. Education on other levels probably differs in economic return. Finally it is important to keep I mind that the wealth score is made relative to the rest of the population. So I have not found the effect on the absolute wealth, but relative to the rest of the population.

7 Figures

Figure 1




Secondary conditional on age1980 male urban
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age1980

8 References

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9 Appendix

Table A1 Self-employed - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) OLS	(2) Probit	(3) 2SLS	(4) IVprobit	(5) 2SLS	(6) IVprobit	(7) 2SLS	(8) IVprobit
Secondary school - full sample	0.00679 (0.0105)	0.00765 (0.0105)	-0.131** (0.0528)	-0.127*** (0.0479)	0.0403 (0.206)	0.0428 (0.201)	0.0402 (0.107)	0.0371 (0.104)
Observations	7605	7605	10925	10925	10925	10925	7605	7605
Secondary school - in phasing period excluded	0.00612 (0.0117)	0.00695 (0.0116)	-0.129** (0.0509)	-0.124*** (0.0459)	0.0667 (0.214)	0.0676 (0.205)	0.102 (0.119)	0.0899 (0.115)
Observations	6438	6438	9227	9227	9227	9227	6438	6438
Controlling for Age	YES	YES	NO	NO	YES	YES	YES	YES
Controlling for Marriage	YES	YES	NO	NO	NO	NO	YES	YES
Controlling for Survey year and Region	YES	YES	NO	NO	NO	NO	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2 Self employed - IV with age in 1980 as instrument for secondary school, standard errors are clustered at the cohort-region level

	(1) only female rural 2SLS	(2) only female rural IVP	(3) only femal urban 2SLS	(4) only femal urban IVP
Secondary school -full sample	0.0548 (0.118)	0.0528 (0.115)	-0.0536 (0.210)	-0.0688 (0.206)
Observations	5139	5139	2466	2466
Secondary school - in phasing period excluded	0.0863 (0.122)	0.0741 (0.118)	0.0768 (0.318)	0.0566 (0.314)
Observations	4347	4347	2091	2091
Controlling for Age	YES	YES	YES	YES
Controlling for Marriage	YES	YES	YES	YES
Controlling for Survey year and Region	YES	YES	YES	YES

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$